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# THE
# SCIENCE OF DISINFECTION.

BY

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GLASGOW.

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## THE

# SCIENCE OF DISINFECTION.

IN the midst of the confusing diversity of opinion among scientific men on disinfection and the agents capable of attaining it; in the midst of so much talk and conjecture made by both medical and non-medical men in the public journals; in the midst of the deplorable confusion between disinfectants, antiseptics, and antizymotics; in the midst of the singular facility with which it is given and received that a disinfectant *par excellence* is a certain mixture which does away with, or masks, the odour of putrefaction or other bad quality;—there is truly much need of receiving something less uncertain on this question, something which will be the result of severe comparative experiments, and not conceptions found or adduced from the property of impeding the putrefaction of organic substances, or of arresting or neutralising it, or neutralising or masking the smell.

These words are quoted from a review in an Italian journal, *La Nuova Liguria Medica*, of a short paper of mine, which appeared in the *Lancet* about twelve months ago, on Carbolic Acid and Zymotic Disease; and I am sure that anyone viewing the question disinterestedly and from a scientific aspect will readily endorse the writer's sentiments.

With the object, therefore, of offering something less uncertain on this subject, I shall presume to lay before you some results of, and deductions from, experiments lasting some years and entailing much close attention and trouble. Many of you must have observed that the words putrefaction and fermentation are frequently used as synonyms, implying thereby a belief in the identity of the putrefactive and fermentive processes, though they are sometimes employed as distinct terms without their difference being defined. Now, I think it can be shown, that whatever be the ultimate changes produced in a putrefying or in a fermenting fluid—and these are apparently the

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same—putrefaction and fermentation are not identical, and that this point constitutes the foundation of scientific disinfection.

If an aqueous solution of fresh beef juice be set aside for some hours and then examined microscopically, it will be found swarming with bacteria, vibriones, amoebæ, &c., in active motion. The fluid, hitherto clear and odourless, soon gets opaque and fetid. In this instance its reaction is of course neutral, but the result is the same were it moderately alkaline or very faintly acid. Furthermore, if the solution is put in a cool and dark place with exposure to air, it will be found at the end of from nine to twelve months still swarming with life, still cloudy and fetid, still giving an appreciable precipitate with nitric acid and heat, though these conditions are now less marked than at first. In a few months later putrefactive reduction is complete; the solution is still cloudy, but now free from putrid odour; a quantity of sediment lies at the bottom of the vessel; a stray microzyme is only seen here and there; it now gives a mere trace of precipitate with nitric acid and heat; and its specific gravity, at first perhaps about 1·6, and which has been gradually lessening, is now probably 1·1, or at most 1·2. Such are the physical characteristics of simple putrefaction; but there being so many kinds of organic matter, one or more of these appearances may accordingly be subdued, absent, or altered. For example, fresh healthy human urine, or hay infusion, soon teems with microzymes; both get clouded, but neither responds to the tests for albumen. The urine emits a moderate fetor, and, at first acid, gets alkaline; but hay infusion keeps comparatively sweet, and, at first neutral, remains so. Then, as is well known, putrefying organic matters yield very various offensive effluvia, according to their constitution and the surrounding medium. There is one condition, however, which is constant in all spontaneously decomposing organic substances, viz. the presence of microzymes. These organisms are certainly the first, last, surest, and in weak solutions frequently the only sign of true putrefaction, the faintest degree of which is coincident with their visible existence—developing or generating them, enduing dead matter with life, or kindling the tiny dynamics of vitality dormant in their as yet hypothetical germs. Some consider them the cause, others the result, of putrefaction; while some hold their function in nature as malignant, others as benign.

So much for putrefaction; and now as regards fermentation and disinfection. If to a portion of the same beef juice solution a quantity of a mineral, or any of the more common vegetable acids, be added so as to cause 'marked acidity,' the mixture will be found in a day or so to contain one or more small tufts of mycelia, their numerous filaments, partly dissepimented from fructification, radiating from a single nucleus, like those of thistle-down; also free spores and torulæ. If the proper quantity of acid have been added, no microzymes are present, and the fluid, at first slightly hazy, soon gets clear; it has of course an acid reaction, a not unpleasant mouldy aroma, and in about four months, as compared with the putrefying fluid previously noticed, ceases to give a precipitate with nitric acid and heat. Supposing, however, that the solution is only rendered moderately acid, then it will ferment at first as before, but in a short time (commonly a few days) it also putrefies, and the two processes continue for a brief period. Latterly fermentation ceases, putrefaction continues, and the fluid is now neutral. Again, if to a portion of putrid beef juice a small amount of acid be added, then in a few minutes the contained microzymes die, and in a day or two minute fungus tufts are seen in the fluid, which now gradually ceases to putrefy, and continues to ferment till the process is exhausted. Again, if to a portion of putrid beef juice a slight excess of acid be added, the mixture is soon odourless, or smells like hawthorn blossom, or spiræa ulmaria, is nearly translucent, free from microzymes and fungi, but gives a precipitate with nitric acid and heat, and remains so indefinitely.

Furthermore, if to a portion of fresh or fermenting beef juice a larger quantity of acid be added than would cause and sustain fermentation *per se*, the mixture neither putrefies nor ferments; it keeps clear and odourless, responds to the tests for albumen, and continues thus indefinitely. (It may be here stated that the acid chiefly used in these experiments was hydrochloric.) The foregoing remarks do not, of course, apply to solutions of beef juice only, but to all fluids containing animal matter. (Specimens shown.) Now, these results are to me irrefragable proofs that putrefaction and fermentation, however correlated, or whatever ultimate changes they produce on animal fluids, are distinct processes. There are no two diseases of the human body with symptoms more opposite, definite, and diagnostic; there are no two pathological changes more different or pronounced; and whether their phenomena be considered from a chemical or physical aspect, they are found absolutely diverse. Such being the case, the question suggests itself, is organic matter as detrimental to health when fermenting as when putrefying? There is, unfortunately, in every town and village in the land abundant evidence showing that putrefying matter is very injurious to health, while we have no opportunity of ascertaining the effects of fermenting matter in this respect. Because putrefaction is natural and spontaneous, but fermentation, as explained, is artificial and involuntary. Judging, however, from the manifestations of the two processes, it is obvious that fermescence must be harmless as compared with putrescence, and their æsthetics as different as their influence on health. Putrid matter evolving noxious effluvia for nearly twelve months, cannot but be more hurtful than fermenting matter, almost odourless, and being fully decomposed in about four months. Nor are minute fungi any more than bacteria, proved to be hurtful when inhaled or swallowed; and those present in fermenting organic solutions are the same as those that grow in cheese, milk, fruits, or other acid animal or vegetable substances in a state of decay, and which are swallowed with impunity. Furthermore, there are many who hold, and I think rightly, that the specific toxic principles of at least typhoid fever and diphtheria are generated *de novo* from putrefying animal matter, which all zymotologists freely admit forms a suitable soil for propagating the whole genus zymotica. Now the disinfecting action of the stronger acids is undoubtedly in many cases owing to their property of substituting fermescence for putrescence, this being the sure result of their moderate use in disinfection. But, as already stated, these acids when added in excess prevent and arrest fermentation and putrefaction. Their action I hold to be thus destructive, in the sense that it dissolves and annuls by causticity the structural configuration of the organic texture or molecules, which being further immersed in an acid medium is thus rendered incapable of spontaneous decomposition. The indication of albumen in the fluid by the subsequent addition of nitric acid is less pronounced than when the mixture is newly made, suggesting the probability that it would ultimately cease to coagulate on the destructive influence being prolonged.[1]

This corroding action of acids on organic substances is, in a hygienic sense, obviously better than the fermentive action; and fermentation, in the same view, is again better than antiseption. If organic matter be fermented, it is probably rendered incapable of engendering or nurturing zymotic poisons; if cauterised, it is amorphosed, in the sense already alluded to; but if antisepted, it is preserved. Now the antisepting of effete nitrogenous substances, excremental or otherwise, has of late been the chief point aimed at in disinfection, and I concede that, as regards fresh animal matter about to putrefy and taint the air, it is beneficial. But what is the consequence if it be practised where a zymotic poison is present, say on an enteric or choleraic stool, or in an atmosphere contaminated with the infinitesimal toxic entities of diphtheria, variola, or typhus? Why, of course, that the poison is preserved as well as the offending matter. The multiplication of zymotic units may be arrested, but the virulence of those already generated or matured, is conserved. What prevents the molecular disintegration of the one, hinders also that of the other. Thus antiseption is seen to be an admirable contrivance for maintaining a constant supply of zymotic germs, or pathoplasts for future epidemics.

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[1] I have now found such is the case.

This fallacy seems to rest on the assumptions that contagia or infecta may arise *de novo* from, and are propagated by, decaying animal matter, and that because antiseption arrests decay it also renders these harmless. Hence carbolic acid has been largely used in smearing the bodies of small-pox patients; it has been kept vapourising in hospital wards, sick rooms, and other infected places; during the rinderpest cloths saturated with its solutions were hung over the horns of cattle, and their stalls and sheds coated with it; it has been given internally both to man and the lower animals for zymotic disease; it has been put into the bed-pan about to receive typhoid and other excreta; the bed clothes from fever patients have been held as harmless when soaked in its solutions; in short, it has been implicitly relied on as a prophylactic of, and antidote for, every phase and kind of contagia and infecta.

Now there are no valid grounds either on logic or fact to assume, as is constantly but I think unconsciously done, that because carbolic acid can prevent or arrest putrefaction, it can also annihilate or annul zymotic poison. To quench decomposition is one thing, and to kill infection another. Sequelæ are not necessarily affected like their cause. As common salt differs in its properties from sodium and chlorine, and hydrocyanic acid from hydrogen, carbon, and nitrogen, so zymotic poisons differ from merely putrid matter. What affects sodium or chlorine separately, does not necessarily affect in the same way common salt; and what affects isolated hydrogen, carbon, or nitrogen, does not therefore similarly affect hydrocyanic acid. Hence, what affects decomposing organic matter does not, as a consequence, in like manner affect zymotic poison. I now proceed to show that contagia and infecta are probably preserved rather than destroyed by carbolic acid, and also to point out some additional facts which indicate a scientific and reliable method of rendering these inert.

1st. A saucerful of pure carbolic acid liquefied by a minimum of water was enclosed within a glass bell-jar of one cubic foot capacity. Twelve hours after, when the jar was filled with strong carbolic emanations, a slip of glass with a minim of vaccine lymph on its surface was placed in the centre of the jar, where it was kept for 36 hours along with the saucerful of acid. The lymph was now moistened with water and glycerine and the reaction found to be neutral; it was then sealed in capillary tubes, and in a few days a child successfully vaccinated with it. Several tubes were filled from the resulting vesicles.

2nd. The lymph from one of these tubes was mixed with ♏i. of a 1 in 50 aqueous solution of carbolic acid ($\frac{1}{50}$ gr.), allowed to dry, exposed to common air for 10 days, moistened with water, reaction neutral, and a child successfully vaccinated with it. Vaccinations with the lymph from the resulting vesicle all succeeded.

3rd. A tube of vaccine lymph was mixed with ♏ii. of a 1 in 20 aqueous solution of carbolic acid, equal to $\frac{1}{5}$ gr. of pure acid. The mixture was milky from coagulation of the lymph. It was at once sealed in tubes. Five days after a child was vaccinated with it. A week after the operation was seen to be unsuccessful—the scratches on the child's arm being quite healed.

4th. A tube of vaccine lymph was mixed with ♏ii. of a 1 in 20 aqueous solution of carbolic acid, equal to $\frac{1}{5}$ gr. pure acid. The mixture in this instance, instead of being sealed in tubes, was exposed on a slip of glass for 14 days, and a child successfully vaccinated with it.

5th. I repeated this experiment, excepting that the carbolized lymph was exposed only 12 days. Vaccination was again successful.

6th. Separate portions of vaccine lymph were exposed to various volatile media in the same manner as in the 1st experiment, but only for 24 hours. They were then liquefied with glycerine, the reaction of the mixture ascertained, and sealed in tubes till children were vaccinated with them. The results showed that with the mixtures of lymph and glycerine which were neutral or alkaline, vaccination was successful, while with those that were acid it was unsuccessful. These experiments were repeated with the acid bodies only; but the acidized lymph, instead of being sealed in tubes, was exposed to the air for about 12 days in order to see whether, as was the case with carbolic acid, the infecting power of the lymph was merely suspended. The results, however, were the same as when the lymph was sealed in tubes. The following is a tabular form of these :—

*Results of vaccinations with Lymph exposed to the vapours of the undermentioned bodies.*

| Successful | Re-action of Lymph and Glycerine | Unsuccessful | Re-action of Lymph and Glycerine |
|---|---|---|---|
| Carbolic Acid | Neutral | Chloride of Lime | Acid |
| Chloroform | Alkaline | Sulphurous Acid | ,, |
| Camphor | ,, | Nitrous ,, | ,, |
| Sulphuric Ether | ,, | Glacial acetic ,, | ,, |
| Iodine | Neutral | Hydrochloric ,, | ,, |

Regarding the 1st of these experiments, I wish you particularly to bear in mind this simple but important truth, namely, that the infecting property of a minim of vaccinine is unimpaired after being buried for 36 hours in the heart of a cubic foot of concentrated carbolic vapour.

In the 2nd experiment, ♏i. of a 1 in 50 aqueous solution of carbolic acid mixed with a tube of lymph and exposed 10 days has no effect on the physiological property of the lymph. In the 3rd experiment a small tube of lymph mixed with ♏ii. of a 1 in 20 aqueous solution of carbolic acid, that is a saturated solution, the acid present being equal to $\frac{1}{5}$th of a grain, and the mixture sealed in tubes for 5 days, failed to vaccinate.

The 4th and 5th experiments are repetitions of the 3rd, excepting that the mixtures of lymph and acid were exposed to common air for 14 and 12 days respectively before being used to vaccinate, which was done successfully.

The results of the 4th and 5th experiments show that even when lymph is incorporated with what is manifestly a large quantity of acid (nearly equal parts), its infecting property is but suspended, and that too only when the mixture is hermetically sealed from the atmosphere, as in the 3rd experiment. Whereas, in the 4th and 5th experiments, where the mixtures were exposed and the acid allowed to vapourise, the lymph, as stated, was normally active.

Now, when vaccinine is thus so obviously unaltered by carbolic acid, and remembering that vaccinine is inimical to varioline, if we are to be allowed to reason at all, it seems a just conclusion that, at least to a similar degree, will varioline be unaltered by carbolic acid, and if so, I submit there is a strong presumption that all zymotic poisons will, under the same circumstances, remain active; and not only so, but that carbolic acid rather antisepts—rather preserves than destroys their infecting powers. Moreover, it will be conceded that the conditions of the experiments were highly in favour of carbolic acid, the proportion of that substance present being greatly in excess of the quantity which it is possible to use in practical excremental or aerial disinfection.

According to a short paragraph in the *British Medical Journal* of Feb. 21, 1874, these experiments and views have been confirmed in a rather remarkable manner. It reads thus: —'It is stated in American medical journals that so signal was the failure of carbolic acid as a preventive of yellow fever in New Orleans and Mobile, that suspicion was awakened that its effect was positively injurious, and that it helped to spread the disease.'

Vaccinine, however, as I have shown, can be rendered inert by exposure to certain acid vapours for 24 hours (though I believe a much shorter time would suffice) and otherwise under the same conditions in which it is unaffected by mixture with carbolic acid vapour for 36 hours. All true science harmonises, hence this fact might be inferred from the influence of acids on organic matter already adverted to. In this instance I am disposed to consider their action destructive, as the moisture in lymph is insufficient to permit of fermentation. These vapours, as seen in the table, are sulphurous, nitrous, acetic, and hydrochloric; the chlorine from chloride of lime producing also hydrochloric and hypochlorous vapour. Now, when vaccinine can thus be made inert, I think there are strong reasons for concluding that so will variola, because as vaccinia prevents variola, so that which destroys the infecting power of vaccinia must, of necessity, destroy the infecting power of variola; and moreover, I hold it would be justifiable, in practice, to extend these inferences so as to include all infecta and contagia. Unfortunately, in the present condition of our knowledge, such conclusions cannot be tested practically, we dare not experiment on an unvaccinated person with variolous matter destroyed by inference only; and, as you are aware, the specific toxic principles of the other zymotica have not been isolated.

There are many more bodies, such as acid salts, which are probably antizymotic from their acid reaction; but as this quality is generally weakened by combination with a base, the acids *per se* should always be used. It so happens that hydrochloric acid, besides being as effective as any, is the cheapest of all acids. This substance, as you know, may be had in any quantity, at a price next to nothing. The commercial acid, diluted with about 50 parts of water, is a fair strength for general disinfecting or antizymotic purposes, though it may be used stronger. In all cases of zymotic disease under my care, I advise that the patient's clothes be steeped for 12 hours in such a solution; and in typhoid fever that a portion of the same fluid be put in the vessel which receives the excreta, and before this is disposed of, that it be mixed with more of a stronger acid solution, also that a portion be sent down the closet soil-pipe afterwards. Hydrochloric acid, unlike nitric, has almost no action on lead in the cold.

I will here digress shortly to notice a substance which is very generally recommended as a disinfectant, more especially of typhoid and choleraic excreta, namely, ferrous sulphate. Supposing you add to separate portions of healthy and enteric fæcal matter a little of an aqueous solution of this salt, you would in the first instance find no change in the aspect or odour of the mixture, while the typhoid excrement would have become a dirty black and free from putrid odour. Now I apprehend it is in consequence of such alterations that the use of ferrous sulphate is advocated in disinfection. What, then, is the rationale of these phenomena? It is this—sulphuretted hydrogen is the chief cause of the fetor of healthy and putrid excrement; at any rate I am sure that bodies which decompose sulphuretted hydrogen can be made to deodorise such matters. But putrid enteric, and stale excrement also contain ammonia, with ammoniaco-magnesian phosphate. Now hydrogen sulphide and ferrous sulphate are only mutually decomposed in alkaline solutions; hence as healthy fæces are acid, no decomposition or deodorisation takes place when ferrous sulphate is added, whereas both ensue with typhoid and decomposing healthy fæces, these being alkaline as already stated. (Experiment shown.) Sulphate of iron, therefore, is not a deodoriser in every case; the medium in which it is to act must be alkaline of itself, or made so. But, moreover, the mere removal of fœtid odour from putrid matter does not imply the destruction or disarming of infecting particles. I have found that ferrous sulphate retards putrefaction to a fair degree, and I think it does so chiefly by appropriating the atmospheric oxygen that would otherwise combine with the organic matter, also by being slightly acid, and not by any caustic action. For the latter reason I should not consider a typhoid stool rendered innocuous, however black or inodorous it had been made by ferrous sulphate, until it was well drenched with a strong acid. In short, sulphate of iron is practically merely a deodoriser, and that only in alkaline solutions.

The experiments with vaccine lymph also prove that to aerial disinfection acids are effectively, largely, and easily applicable; hydrochloric, acetic, sulphurous, and nitrous acids being volatile, plentiful, cheap, and vapourised with facility. In practice I usually advise several platefuls of chloride of lime to be placed in various parts of an infected house, and the ignition of greater or less quantities of sulphur, as the case requires or admits of. Also, that the patient's body be sponged daily with an acid lotion, and acid drinks freely allowed.

I shall now very briefly refer to the action of alkaline substances on animal matter. In the course of the protracted series of experiments already alluded to, and which were made to determine the relative antiseptic powers of above 60 substances, as evinced by their preventing the appearance of fungi and animalcules in organic fluids with which they were mixed, I frequently observed that several of these bodies seemed rather to accelerate than retard the beginning of putrefaction. It was also noted that their solutions were either alkaline or neutral, from the bodies themselves being either alkalies, or alkaline earths, or their salts. Further investigations confirmed these results, which are in accordance with the fact mentioned by Dr. Gregory in his 'Organic Chemistry,' Ed. 3, 1852, p. 32, that bodies with an alkaline reaction, to which I may add neutral salts of the alkalies and alkaline earths, when mixed in small proportion with organic matter, hasten its decomposition—a fact which should be remembered by those who recommend the mere dusting of lime on open drains.

Such, however, is not the result if excess of caustic potash be added to a recent solution of animal substance; for although there is an almost instantaneous evolution of putrid odour, yet in a comparatively brief period it disappears, while the fluid remains free from fungi, fœtor, or microzymes, and soon ceases to give a precipitate with nitric acid and heat. Again, if excess of potash be added to a putrid solution of beef juice its microzymes quickly die; its bad odour, after a lengthened period, is gone; while its turbidity is changed to transparency, and it now gives no response to albumen tests.

Regarding the action of alkalies on vaccinine, which substance is itself alkaline, I have only three experiments to submit.

1st. Two tubes of vaccine lymph were mixed with mss. liq. pot., B.P. Twenty-four hours after the mixture, now dried into a film, was moistened with water, and a child vaccinated with it. The operation was *un*successful.

2nd. Two tubes of lymph were mixed with mss. of a mixture consisting of 1 part of liq. pot. in 20 of water. One day after the residual film was moistened with water and a child vaccinated with it. The operation was entirely successful.

3rd. Two tubes of lymph were mixed with mss. of a 1 in 20 solution of liq. pot. and water. The mixture was laid aside for 10 days, to see whether more prolonged contact of the potash and lymph would annul the latter's infecting powers, in accordance with the fact that alkalies hasten the oxidation of organic matter. The remaining film was moistened with water, and a child vaccinated with it. The operation was *un*successful.

The 1st of these experiments shows that the infecting property of lymph may be quickly destroyed by mixture with an excess of potash.

The 2nd proves that a moderate quantity of potash mixed with lymph does not soon annul its infecting powers.

The 3rd. shows that if the conditions of the second are prolonged for 10 days, the lymph is made non-infective. From these results it may be justly concluded that variolous and other zymotic poisons would in the same circumstances be affected similarly to the lymph; also, that the other alkalies would act on lymph similarly to the potash, and in like manner on the other zymotica.

The action of acids and alkalies on organic matter may be summed up thus :—Acids added in small proportion to a fresh solution cause it to ferment, and prevent it putrefying. Added in large proportion, they prevent both putrefaction and fermentation. Added in small proportion to a putrid solution, putrescence is arrested and fermescence induced. Added in large proportion to a fermenting or putrid solution, both processes are arrested indefinitely. Vaccinine is quickly made inert by acid vapours.

Alkalies added in small proportion to a fresh solution, hasten putrefaction and prevent fermentation ; added in large proportion, they prevent both fermentation and putrefaction ; added in small proportion to a fermenting solution, fermescence is arrested and putrescence induced. Potash added in small proportion to putrid solutions, exacerbates putrescence ; added in excess, putrefaction is soon expended. Potash mixed in large proportion with vaccinine, quickly makes it inert; mixed in moderate proportion and for a short time, it remains active ; mixed in moderate proportion for 10 days, it ceases to infect.

In conclusion, it may be stated that special reference to the various doctrines of the immediate causes and nature of putrefaction and of zymosis, and also regarding the *modus operandi* of many well-known disinfectants, has hitherto been purposely avoided. These, though very interesting, consist chiefly of hypothetical data, so abstruse and complex that their consideration would in all probability contribute to no practical result. Nevertheless, this paper would be somewhat incomplete were the apparent modes of operation of disinfectants not, at least, briefly noticed. These I endeavoured to classify, but no arrangement I have made or seen has satisfied me. The following is that of Dr. Letheby submitted to a meeting of medical officers of health held in London last October. He divides disinfectants into four classes, to use his own words, 'according to the manner in which they seem to destroy or prevent the hurtful thing which causes offence or produces disease.' As examples of the first class, he alluded to the effects of cold and dessication, which strengthen the affinities of organic substances, thus enabling them to resist decay. Second. To the coagulation of albuminous matters by alcohol, creosote, the mineral acids, and most mineral salts which thus produce new compounds, not susceptible of organisation or decay. Third. To the oxidizing effects of chlorine, hypochlorous acid, permanganate of potassium, and atmospheric air, aided by water and porous bodies, which hurry on the changes of decomposition and oxidation, so that the particles may quickly arrive at the final stages of decay, and be quickly brought to rest. Fourth. To sulphurous, carbolic, and cresylic acids, and to the volatile oils which are found to have specific powers of disinfection.

I shall not discuss this arrangement further than to remark that the majority of such bodies are capable of acting in more than one of these ways. Also, that the action of a disinfectant depends in some degree on the proportion in which it is mixed with organic matter. But, assuming the relative disinfecting powers of the bodies mentioned as ascertained, the prior statement still holds good. Numerous examples might be cited. I shall give one. Chromic acid is a powerful coagulator of albuminous matters; it has a strong affinity for water (which, I may remark, is considered the *modus operandi* of antiseption by common salt); it is also a strong oxidizer, and therefore deodorant.

I am aware the imperfections of this communication are numerous, for which I plead the great difficulty of the subject, which, however, I trust my remarks will tend to lessen. Two points are worthy of reiteration. 1st. Putrefaction of organic matter may be impeded, arrested, or neutralised, or the odour neutralised or masked, and yet any zymotic poison present, in all probability, be unaffected, conserved, or only made dormant for a short time; in other words, the experiments with carbolic acid and vaccinine show that the use of pure antiseptics as antizymotics is a palpable paradox, preservation being practised and destruction expected.

2nd. The mineral acids are true disinfectants, they prevent putrefaction, they arrest putrefaction, they transform putrefaction into fermentation, they deodorise, and, what is most important, they are highly antizymotic as regards vaccinine, and therefore *à priori* of other contagia and infecta.

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